ANALYSIS

MR. WILLIAMS ON THE A PRIORI [1]

By D. A. T. GASKING

I WISH to comment on one central contention of a paper by Mr. D. C. Williams on " The Nature and Variety of the *A Priori* " (*Analysis* : Sept. 1938).   Mr. Williams is there concerned to refute in its various forms, what may conveniently be called the " conventionalistic " theory of the *a priori*.   After dismissing various cruder forms of this theory, he proceeds to criticize what he calls a " milder" form, which may be formulated as follows : " An a priori proposition is such that its truth depends wholly on conventions, or on the way in which words are used."

Against this Mr. Williams wishes to urge that for both *a priori* and contingent statements " the truth of the statement depends primarily on a connection among what is meant, and only incidentally on the convention which determines the mode of expression of this connection" (p. 87).   Using ' N ' to stand for a necessary proposition and ' C ' for a contingent one, Mr. Williams argues : " All that fiat or convention can determine is whether and how we shall symbolize the truth, not the truth itself . . . Some of the persons who have claimed that the truth of N depends wholly on rules of language have no doubt laboured under the misapprehension that such rules are connectional or syntactic definitions . . . this is not the case . . . the words in N have their semantical or applicative meaning, just as do the words in C, and whatever special connection subsists

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[1] My debt to Wittgenstein in writing this paper is enormous, but he should not be assumed necessarily to approve of any part of it.

among the words, either of N or of C, is derivative of the connection among what they mean." (p. 87-88.)

If I have understood him correctly, Mr. Williams' argument might be stated in more detail as follows : Conventions about or rules for the use of language may be either syntactical or semantic. If a rule is purely *syntactical* it will specify what words or sentences may or may not be substituted for each other, or in what ways certain words may or may not be combined with each other in sentences. It will not, however, say anything about the way in which words may or may not be applied to reality. Thus a syntactical rule for the use of the symbols ' X ' and ' Y ' would be: " No sentence may be constructed of the form : ' Something is both ' X ' and Y '." Nothing is here said of what objects or properties X and Y are to be used to stand for. Such a rule might be part of a " pure uninterpreted logistic system ", and would be in many ways like a rule of chess. In this case our choice of this rule rather than its opposite is clearly arbitrary: we might very well have constructed our "uninterpreted" language in such a way that the rule was : " Sentences of the form ' something is both X and Y ' are permitted." But *having decided* on the rule excluding " both X and Y ", it will, in our uninterpreted language, be a necessary proposition that " nothing can be both X and Y." If, however, we had chosen the opposite rule, this proposition would not have been necessary. But this freedom of choice obtains only where the rules are purely syntactical, which the rules of English are not.

A *semantic* rule, on the other hand, prescribes what objects (properties, events etc.) the symbols X and Y may correctly be applied to. Such a rule, for example, might lay down that X was to be applied only to round things and Y only to square things. (These rules, of course, need not be given by the use of any symbols other than those to be explained ; it could all be done by " ostensive definition "—pointing and saying ' X '). Now, if such semantic rules for X and Y are given, they will determine what objects X may be applied to and to what Y may be applied. They will thus determine *incidentally* whether X and Y may both be applied to the same object. But this will *primarily* depend on whether or not it is *possible* for the characteristic determining the application of X to qualify any particular qualified by the characteristic determining the application of Y—that is it will depend

primarily on " a connection among what is meant ".   Thus if it happens to be in fact impossible for things to be both X and Y it will be a syntactical rule that " no sentence may be constructed of the form : ' something is both X and Y '," and it will be a necessarily true proposition that " nothing X is Y."   But neither this rule nor the necessary proposition is arbitrary, a matter of our convenience ; it is unalterable and fixed by the nature of the world.   If you do change such a syntactical rule you can only do so by changing the semantical meaning of the symbols involved, e.g. you may admit the sentence " this is both square and round ", only if you change the use of " square " so that it stands for what we now call " red ", or make some similar semantic change. Similarly you may change the syntactical rule " Q is a consequent of P " only if you change the semantic meaning of either P or Q.   " The only sense," says Mr. Williams (p. 93) " in which . . . we can change piecemeal the consequents of any statement, is that whereas a sentence like ' Books are rectangular ' has one set of consequents on the ordinary interpretation, it has another set if somebody uses it as a code-phrase for e.g. *If the enemy attempt to evacuate we shall attack at once*."

I hope this is a fair account of Mr. Williams' position.   I now want to ask : if the connection between the words of a true proposition, necessary or contingent, is determined by or " derivative of " a " connection among what is meant ", what is it that determines what is meant by each separate word ?   Presumably Mr. Williams would say " the semantic rules for that word."   But which semantic rules, and what are they like ?   Of such rules connecting up symbols with reality by prescribing under what circumstances they may be used and under what they may not, there is a great variety.   I will describe some types :

(1) Suppose you are teaching English to a child or to a foreigner by the " direct method " (i.e. without using any language but the one taught).   You might start by pointing to something round, say a penny, and saying " Round ! "   This would be a part of the process which we should call teaching him the semantic rules for " round " (i.e. teaching him the meaning of " round ,") and thus *one* of the semantic rules for this word is that it is to be applied to the object pointed to, namely the penny.   (This will be called an " ostensive definition ".)

(2) Simply doing this would probably be insufficient to make

him understand the word. Whether he has understood it or not we test by seeing how he goes on to apply the word in other cases. Thus he might in future (a) apply the word " round " only to the penny you have pointed to and to nothing else. In this case we should say he has interpreted the word as a proper name for that penny. (b) He might apply the word to the penny shown and to other pennies, but to nothing else (he interprets " round " to mean " penny ") ; (c) he applies it to the original penny, to other pennies, halfpennies, shillings, etc., and to nothing else (he interprets it as " coin ") ; (d) he applies it to the original penny, and to other pennies and halfpennies (but not shillings) and to brown curtains, shoes, withered leaves, etc. (here we should say he interprets it as " brown ") ; (e) he applies it as we do the word " this ", or like the word " here ", or like " how nice ! " ; (f) he goes on to apply it as we do. To make sure that he goes on in this last way, and not in any other, we might point to twenty or thirty round objects, differing in other respects, and each time say " Round ". Thus we would give him twenty or thirty different rules for " round ", each one analogous to the rule in case (1).

(3) We introduce the expression " not round ", and by a large number of examples as above we teach him when to say " round " and when " not round ". Thus we give him, say, twenty particular rules for " not round ", and a few more for " round ". By this time he will probably have learnt the word ; will understand it. By his having learnt the meaning of the word we mean that he now knows the semantic rules for its use. But have we taught him *all* the rules ? We have taught him how to use it in, say, fifty particular cases. Have we thereby taught him how to use the word in cases which fall outside this class ? We have not done so, in the sense that we have not shown him these other examples. We have probably done so and he probably now knows all the rules in the sense that he will now use the word, if faced with these other examples, in the way we should, and in the way we should call correct. But it is always conceivable that, even after he has in a great many fresh cases used the word in such a way as to lead us to say he had understood it and learnt the rules for its use, he should suddenly in another new case so use it as to lead us to say : " So he doesn't really understand it at all."

(4) We teach him a number of other adjectives, e.g. " square ",

" red ", " heavy ", " hard ", etc., and the corresponding negatives " not square ", " not red ", and so on, in the manner described above. We now teach him the rules for " and " and for " not both . . . and . . . " by teaching him in what circumstances to use " red and hard ", etc., and in what circumstances not to use this or to use " not both red and hard ", etc. In this case again, just one example with " red and hard " will probably teach him when and when not to say " red and hard " in future cases, but it might not, and this one example may or may not teach him how to use " round and hard ", " hard and heavy " and so on. If we find that he does not go on in the right way we shall conclude that he hasn't understood and give him more examples until he does. Now, doing this is not only giving rules for " and ", it is also giving *new* rules for " square ", " red ", and so on. It is only probable, and not certain, that a person who has learnt " red " and " round " as in 3 above, and has been taught " and " with various examples which did not happen to include the particular example " red and round ", will in fact use that expression correctly when it comes to the point. (Whether we say, if he does not, that he has misunderstood " and ", or misunderstood " red " or " round ", or all three words, will depend on the circumstances.)

(5) Someone after the above teaching calls something (say an octagon) " square and round ": we tell him " nothing can be both square and round ", i.e. we teach him that rule for the use of the words " square ", " round " and " and " which lays down that *nothing* is to be called by this particular group of words. This could be regarded as a special " limiting " case of the type of rule described in 4. Those rules lay down in what circumstances a word-group is to be used, in what case not. This sort of rule lays it down that the word-group is to be used in *no* circumstances whatever. This rule too is a semantic rule for the use of the words involved for (a) if someone breaks it, this shows that he is using at least one of the words involved in an incorrect way ; (b) if the rule were different we should say that the semantic meaning of at least one of the words was different. (Cf. Mr. Williams' remark about using ' Books are rectangular ' with different consequents.) Moreover, rules of the type described here are such that to each there corresponds a necessary proposition.

(6) The rules for the use of the word " same " are taught in an essentially similar way to those for " round ", e.g. a few examples are given by pointing and saying " this is the same as that." Afterwards the pupil probably goes on to apply the word in fresh contexts exactly as we do. But at any time he may not go on as we do, and we would then have to correct him.

(7) The rules for the words " colour " and " shape " are taught, e.g. by saying red, green and blue are colours and not shapes ; round, square, etc., are shapes and not colours.

(8) At a later stage the rules for " the same sense of a word " and " different sense " are given, as in previous cases, by numerous examples.

To this description of the semantic rules of language it might be objected : " These hundreds of particular instances which you describe are not properly speaking rules, but rather applications of a rule. There are not hundreds of different rules for ' red ', corresponding to different ostensive definitions, but *one* rule, of which these are particular applications, and which might be expressed by pointing to something red and saying : ' Red is to be used for anything of the same colour as this. ' " I do not wish to quarrel about a word, so let us call this general rule a " *rule* " and what might be called particular applications, " *rulings* ". But notice that the general rule only functions as a rule if the words in it are understood. It includes in its expression such words as " same " and " colour ", and these words too have to be taught before the rule is understood. Now can the word " same " be taught by a *general* rule of the type described, or must it be taught by means of many rulings? If you can and do give a general rule for it, this too will contain words that have to be taught : sooner or later you would have, in order to explain the word " same ", to resort to a number of rulings. The same applies to the word " colour ". Once certain key-words have been taught by numerous rulings you will be able to short-circuit the explanation of a *new* word by giving as a substitute for numerous rulings a *general* rule in terms of those keywords.

We say that a ruling for a word in a particular case " follows from " or " is determined by " a general rule if people who understand that rule apply the word in that particular case according to that ruling. But the *criterion* for whether a person understands

a general rule for a word is that he always uses it according to such and such rulings—namely the rulings which we say " follow from " it.   What the general rule *means* depends on what rulings we admit as correct applications of it.   The same applies, *mutatis mutandis*, to the determination by a given range of ostensive definitions of the correct way of going on in a fresh case outside that range.

Thus *what is meant* by a word like " round " depends ultimately on the rulings which decide for each particular circumstance whether the word may correctly be applied or not.   And one of the rulings which determine what is meant by the words " and ", and " round " and " square ", and therefore also " the connections among what is meant " by those words, is that which says that the phrase " round and square " is to be applied to nothing at all.   To this ruling there corresponds the necessary proposition : " Nothing can be both round and square ".   If we had a different ruling for this paricular case, this proposition would not be a necessary one.   Therefore, it seems, the " connections among what is meant " are determined by what propositions are necessary, and *not* vice-versa, and this is what the conventionalists assert, and what Mr. Williams is concerned to deny.

Mr. Williams, however, might admit this but deny that it was a relevant objection to his main point.   I will try to state what I imagine this point might be if reformulated to meet what has been said.   The argument might run as follows : Let us call those rulings for the use of words to which there correspond necessary propositions (cf. 5 above) the *a priori* rulings or rules.   Then ; it is admitted that the reason why you cannot change the necessity of propositions without changing the semantic meanings of words involved in them is that the semantic meaning is determined *inter alia* by the *a priori* rulings for the use of those words, and may be defined in terms partly of such rulings.   But what is *not* possible is that you should use words according to all the rulings we at present accept for them with the exception of the *a priori* rulings, i.e. if in other contexts words are used as we now do, we could not do other than use them as we do in the context of necessary propositions—could not make some propositions at present contingent, necessary, and vice-versa.

In reply to this I would ask the reader to consider the following

example. At present we say " a thing can't be both blue and red (at the same time over its whole surface)". This is, we have a ruling forbidding the application of the phrase " blue and red " to anything. Now could not this happen : *everything* that we now call " blue ", " red ", " blue and heavy ", " red and hard ", etc., etc. we call by these expressions, and similarly with their negatives except for one case. The exception is that we call a certain shade (say what we now call " fuchsia ") " blue and red all over at the same time ". If asked of such a colour " Is this blue ? " or " Is this red ? " or " Is this both blue and red" we answer " yes ", and moreover everyone calls this a *correct* answer. Everything else in our language for describing colours works as it does at present, except for this anomaly in the description of this one particular shade. Then it would no longer be a necessary proposition in our new queer language that " nothing can be both blue and red."

A number of objections to this would probably be made and I will deal with a few likely ones in turn.

(a) It might be said : Obviously the case described could happen, but then clearly the words " red and blue " would in the anomalous case be used in a sense different from that in which they are used in other cases. This is a valid objection provided that the expressions " same sense " and " different sense " are being used as we use them at present. So we have to imagine that there is a further anomaly in the rulings for the use of words in the language described, over and above the anomaly which consists in describing a certain shade as " red and blue ". This further anomaly is in the use of " same sense " and " different sense ", which, in the language described, function in such a way that the sense in which " red ", " and " and " blue " are used in describing this shade, is *called* " the same sense " as that in which " red ", " blue ", etc. are used in other contexts. In one particular case we have a different ruling for " same sense."

(b) It might be objected that, if so, the expression " same sense " when used in this anomalous fashion was *itself* used in a different sense from other contexts. So in the language described we have to suppose that with respect to *two* rulings the expression " same sense " is used anomalously; namely in calling the sense of " red " in " red and blue " *the same sense* as in other cases, and also calling the sense of "same sense " in that context the

same sense of the expression " same sense " as in other contexts.

(c) It might be objected that if we called the use of the expression " red and blue " in the above case " correct " we were using " correct " in a different sense from that in which we say it is correct to call a penny stamp " red ". So we have, in our queer language, to have yet another anomalous ruling for " same sense " which says that " correct " in the above is used in the ordinary sense.

There might be further slight modifications along the above lines with respect to our rulings for words involving " same ". These may easily be imagined. The essential point is that what any word means for us, including the word " same ", depends on the particular rulings according to which we use it in all sorts of particular cases. And that we should use e.g. the word " same " exactly as we do in all but a few contexts, and in those contexts use it differently, may be a psychological, but is not a logical impossibility. We could at least *describe* what this would be like.

The foregoing should incidentally throw light on the reason why a proposition which is either necessary or contingent in one language is also respectively either necessary or contingent in any other language into which it is translated. The reason is that our *criterion* for a translation being correct is that the original proposition and the translation should both function in their respective languages in the same way, i.e. according to the same rulings. Thus, if we found that a suggested translation of an English necessary proposition did not function in the new language as a necessary proposition, we should take that as proving conclusively that it was no translation. The same applies, *mutatis mutandis* to contingent propositions. Thus the English necessary proposition " nothing can be both red and blue " cannot be translated into the " queer " language described above : there is no sentence in it which functions as the English one does. And if it could be translated the translation would be a necessary proposition. Similarly, if there is in any other language a necessary proposition, either its English translation is itself necessary, or it cannot be translated. What we *now* mean by " a thing can't be both red and blue " will always remain true however differently we use language. But this does not prove that its truth does not depend wholly on our present linguistic

conventions, or that we could not have different necessary pro-
positions if we had different conventions. We might use a
language in which what we now mean by this proposition could
not be expressed, and in which we expressed propositions
which could not be expressed in English. And if, at any time,
we used such a language, the necessary propositions in it which
could not be translated into English *would always have been true*.

Many other points, for which there is not space, remain to be
dealt with.

ON MEANING AND VERIFIABILITY
By C. H. Whiteley

IT is fairly often said that, apart from "tautologies", only those
propositions which are verifiable can be said to have a meaning;
or, more boldly, that the meaning of a proposition *is* the method
of its verification.   It is generally agreed that there are different
sorts of verifiability—"strong" and "weak" verifiability, veri-
fiability "in practice" and "in theory" or "in principle";
I shall refer to these distinctions in the course of the discussion.
My particular concern is to argue that there are also different
sorts of "meaning", and that confusion between the various
senses of that treacherous word has misled philosophers in the
consideration of this "principle of verifiability".

(1) To say that a proposition (statement, belief, hypothesis,
fact) has "meaning" or "significance" is often more or less
equivalent to saying that it is important, interesting, or fruitful ;
that it matters whether you accept it or reject it (cp. "Darling,
you mean more to me than anything in the world"). This is a
vague use of "meaning", and shades off into the still vaguer use
in which people ask questions about the Meaning of Life and
the like.   In this sense of "meaning", to say that only verifiable
propositions are meaningful is to say that they alone are worth
formulating and discussing, while unverifiable propositions are
barren—it makes (or logically it ought to make) no difference
whether you accept or reject them.   If somebody says "The
Universe is a single substance" one wants to ask "So what ?
What difference does it make whether I say that it is or that it
isn't ?", and to answer "None".   To enunciate the principle
of verifiability is one way of exhorting people to stop bothering
their heads with speculations which they have no means of testing
and to get down to brass tacks ; it is a rather rhetorical way of
doing this because to say that a statement is meaningless is usually
to say something stronger than that it is useless (see below).   I
think that this is in part the intention of people who assert this
and similar principles.   They are urging us to concentrate our
attention on the way in which a proposition can be proved or dis-
proved, and to ignore all those statements (apart from "taut-
ologies") for which we have and can obtain no experimental

backing. If this is so, I think that the exhortation is sensible and salutary. (It will still allow the speculative philosopher to indulge in the formulation of hypotheses which admit of only a remote and uncertain verification). If we interpret the principle in this way, we shall give " verifiability " its ordinary and straight-forward sense, in which a hypothesis is (for me) verifiable when I am able to obtain evidence for it. That is to say, " verifia-bility " here covers the " weak " as well as the " strong " variety —not merely the observing of the actual fact asserted, but the observing of other facts which are evidence for it. Incidentally it also covers falsifiability—evidence against will do just as well as evidence for; clearly when I formulate a hypothesis I do not know whether the evidence I shall obtain will be favourable or not. But it does not cover verifiability " in principle " : a hypothesis for (or against) which there is no evidence is none the less useless because it might have been the case that there was some. Thus one possible interpretation of the principle of verifiability is as a recommendation to ignore as unprofitable those hypotheses which cannot be actually tested. Such a principle, however obvious it may be, is clearly not an *a priori* axiom, but a tactical precept.

(2) We sometimes find " meaning " used in an allied but rather more precise sense, in which the " meaning " of a pro-position (statement, hypothesis—but I think the right word here is " belief ") is the effect which it has upon the behaviour of the believer. Thus, if we say that his religion has no real meaning for X, we are likely to mean that his conduct is not affected by religious beliefs. In this sense the " meaning " of " The house is on fire " to me is the effect produced on my behaviour when I hear somebody say so (and believe him)—e.g. I ring for the fire brigade and grab for the valuables ; and these actions are said to constitute an " understanding " of what he says. This is an outlandish sense of " meaning ", which I mention chiefly to obviate any possible confusion. It is obvious that in this sense of " meaning ", the second and more extreme form of the principle of verifiability is false : ringing for the fire brigade is not in any sense a way of verifying that the house is on fire, though, of course, an attempt to verify a statement may be one of the effects of hearing the statement uttered. The first form is false too ; for people's actions are often affected by beliefs

which are not verifiable in any sense.   We may therefore dismiss this sense of " meaning ".

In both these senses of " meaning ", it is beliefs or hypotheses which " mean ".   Normally, however, it is not beliefs or hypotheses which are said to have meaning, but symbols.   The difference is commonly obscured by the ambiguity of that abominable word " proposition ".   " Meaning " is a state of affairs which involves (a) symbols (b) the interpretation of the symbols—the thinking of the interpreter (c) the facts referred to. The word " proposition " may on occasion stand for any of these, and it may hover uneasily in a vacuum between them.   The queer phrase " the meaning of a proposition " is a prime example of this confusion ; if the proposition is to be distinguished from the sentence or set of sentences, " meaning " must have some such sense as is indicated in (1) and (2).   The more usual sense, in which symbols are said to have a meaning, i.e. to be understood, is quite different.   For a sentence may be meaningful to me even though what it states is of no interest or importance to me, and even though the utterance of it does not affect my behaviour. If somebody says to me " The house is on fire " and I do not believe him and consequently do nothing about it, the sentence is nevertheless understood, and has precisely the same meaning as it has if I do believe him and act accordingly.   To say that it is understood, or has meaning, is to say something about the sentence, and about certain processes of thought which follow upon the perception of the sentence (just what, I have not time to discuss).

There is still another distinction to be drawn, namely that between the meaning of words considered apart from their context, and the meaning of sentences which express assertions or questions or commands.   The difference is sometimes expressed by saying that words by themselves have no meaning : I prefer to be less provocative and say that they have not the same sort of meaning ; a word by itself cannot assert, or inquire, or command anything.[1]   We have to know the meaning (in one sense) of the words in a sentence in order to know the meaning (in another sense) of the sentence, or to know that it is meaningless.

[1] Perhaps this is the distinction Mr. Russell is really referring to, when he talks about the " object language ", in which it is impossible to lie (Arist. Soc. Supp. Vol. XVII, pp. 44ff.). Of course, in any language in which it is possible to make an assertion, it is possible to make a false assertion.

(3) As to the meaning of separate words, it is obvious that words can't be verified. Nevertheless it is not irrelevant to our inquiry to ask the question Under what conditions can a mark or a sound have a meaning ? This is one way of putting a question which greatly concerned Locke and Hume. Their answer is, very roughly, that I can only think, and a fortiori can only talk, about the sorts of things, events, facts, which I have previously observed. Thus, if a word is to have meaning for me, it must either have been ostensively defined, i.e. "applied" to some thing or event or fact which I have observed, or have been defined in terms of other words which have been ostensively defined. (I ignore the snags which arise in connection with the "meaning" of words like "is" and "if". Hume more or less says that "exists" has no meaning ; perhaps he is right.) This doctrine is commonly called Empiricism, in contrast to the "rationalist" doctrine that some words, such as "cause" and "God", have meaning, even though I have never observed an instance of causation or anything having the attributes of deity, nor can these words be defined in terms of anything I have ever observed. Now to say (as the old empiricists said) that I can only talk about the sorts of things I have observed is rather like saying (as the verificationists say) that I can only talk about the sorts of things that are observable ; if an object has been observed, a fortiori it is observable. To verify "There is a horse in the garden" may be to repeat the sort of experience I had when I first "applied" the word "horse" to the sort of object to which I do apply it. Nevertheless, there is a great difference between a theory which asserts the dependence of the meaningfulness of words upon *actual past* experiences, and a theory which asserts the dependence of the meaningfulness of sentences upon the possibility of (presumably future) experiences. The theories are different, but not opposed, for it seems to me that the principle of verification is trying to deal with a different problem, namely

(4) the problem of the meaningfulness (intelligibility) of sentences, i.e. the question In what ways can (meaningful) words be combined so as to make a meaningful statement (or question, or command, though these are sometimes forgotten) ? For the crime of the "metaphysician" is surely that he puts together to form meaningless sentences words which can otherwise be put together to form meaningful sentences. It is

clear that if the empiricist principle is true, one condition of the meaningfulness of sentences is that the words in them should obey that principle.   Another condition, of course, is that they should obey the rules of the language " in " which they are—more or less ; ungrammatical sentences are often intelligible all the same.   But these conditions are insufficient ; they still leave us with sentences like " The binomial theorem is pale green " and " Pain is an illusion ".   It is one of the achievements of the Logical Positivists to have drawn attention to the fact that there are other conditions and that it is important for philosophers to understand what they are.   And the principle of verifiability is, I believe, intended to be an answer or part of an answer to the question How can we distinguish meaningful combinations of words from meaningless combinations ? Now I think it is clear that if we are to say that verifiability is a criterion of the meaning-fulness (i.e. the intelligibility) of sentences, we cannot be using " verifiability " in the straightforward sense which I mentioned in section (1)—verifiability in practice.   " Tacitus often got blind drunk ".   Of this sentence it may be said (a) I am not in possession of any evidence bearing on its truth or falsity, (b) I do not know whether or not there is any such evidence extant, (c) I have no intention of taking any steps to find out.   Yet I know that the sentence is meaningful and not meaningless, and I know what it means.   The meaning of this sentence (in this sense of " meaning ") is not the way in which it is actually verified, be-cause it is not verified ; nor yet the way in which I can verify it if I choose, because I can't (so far as I know) verify it at all.   If " verifiability " means verifiability in practice, this sentence is on the principle of verifiability meaningless.   As a matter of fact it is meaningless in senses (1) and (2) of " meaning ".   If verifiability is to be a criterion of meaningfulness in the sense in which this sentence is meaningful, it must be another kind of verifiability.   It seems to me that there is a test which we sometimes apply (particularly if we are modern philosophers of the sort who read " Analysis ") when we are puzzled as to what a sentence can mean or whether it can mean anything at all, a test for which " verifiability " is a possible though not a very apt name.   This test is not the attempt to verify it—before we can do that we must already know what it means, and if the verifica-tion is to be " weak," as it usually is, we must also know its

logical relations to other propositions, so that we can recognize
that the evidence is evidence when we come across it.   It is the
attempt to think of (conceive ? imagine ?) " what it would be
like " (to use a current phrase) if the sentence in question were
true (" expressed a true proposition ", if you prefer it) ; in other
words, to think of the sort of observations which, if they were to
be made, would be described by the sentence in question.   And
so we say that two sentences mean the same if they could be used
to describe the same observations.   And if we can think of no
sort of observation or observations of which the sentence in
question would be a description, we declare the sentence mean-
ingless (to us, though it may be meaningful to somebody else ;
perhaps some of the sentences uttered by mystics are intelligible
to other mystics, though not to anybody else).   If we then go on
to say that a sentence means (not the observations which do or may
verify it, but) the observations which, if they occurred, would
verify it, we are saying something pretty plausible, and probably
somewhere near the truth.   I am not at present concerned to
discuss how near the truth it is, but to stress the difference
between this sort of " verifiability " and the sort of " verifiability "
to which I referred earlier—a difference so great that I think the
use of the same word in both cases is misleading.   In applying
this criterion we do not require that we should be able to perform
a verification, but merely that we should be able to suppose one.
The " possibility " of verification here is not practicability, but
that sort of possibility which is equivalent to conceivability—
" logical " possibility perhaps, though I do not find the phrase
very enlightening.   I think a good deal of trouble may be caused
in this connection by using the word " possible " without
expanding it.   The conditions of " possibility " here are clearly
much wider than the conditions of practical possibility.   But in
one respect this criterion is stricter than the other.   In order to
understand what a sentence means I must be able to conceive
" what it would be like ", what observations could be made, if
what the sentence asserts were the case, and not merely if there
were some evidence for it—which there might be even though it
were not the case ; still less if there were merely some evidence
against it.   That is to say, the sort of verification which has to be
" conceivable " or " possible " is not merely " weak " verification,
but conclusive verification (I make the statement with some

hesitation, but still I make it).   I think some difficulty may be avoided here if we are careful not to confuse facts which are evidence for a hypothesis, with facts which are part of the content of that hypothesis.   If water is defined as a colourless tasteless liquid boiling at 100° C., then when I assert " This liquid is water," part of what I am asserting is that the tests of looking at it, tasting it, and boiling it will have certain results.   Being tasteless in part constitutes being water.   Thus, though we may say that " This liquid is tasteless " is evidence for " This liquid is water ", the logical relation is quite different from the logical relation between " There is an announcement in the Times to the effect that Mr. Smith has married Miss Jones " and " Mr. Smith has married Miss Jones ", though the former is also evidence for the latter, and would constitute a " weak " verification of of it.

To sum up.   There are uses of " meaning " in which it applies to hypotheses or beliefs, and refers to their " cash value in experience "—their importance to the thinker ; and there are uses of " meaning " in which it applies to symbols, and refers to their interpretability.   There is a sense of " verifiability " in which it refers to the capacity to perform a " weak " verification under existing conditions, which is in point when we are talking about the " cash value " of a hypothesis ; and there is a sense of " verifiability " in which it refers to the conceivability or the " logical " possibility of a " strong " verification, and is in point when we are talking about the intelligibility of sentences.   If these different senses of the words are not clearly distinguished there will be confusion between the question What would be the case if this sentence was true ? and the question How am I to set about verifying this hypothesis ?   It seems to me that this confusion has been committed, and that it is responsible for some of the most objectionable features of Logical Positivism.   For instance, the ridiculous " translation " of statements about the past into statements about the future can only commend itself to a philosopher who is thinking in terms of practical verifiability. If a verification is to be practically possible, obviously it must be performed in the future.   But for a statement to be intelligible, all that is needed is that a verification of it should be " conceivable " or " logically possible "; and clearly past verifications are both possible (some of them have happened) and

conceivable (else there would be no memory). Similarly with
the equally ridiculous analysis of statements about your mind
in terms of statements about my observations of the external
parts of your body. The answer to the question How am I
to test this hypothesis ? must obviously be in terms of my own
actions (I need not perform the actual experiments which verify
a scientific hypothesis, but at any rate I must be able to read the
reports of the experimenters). But the answer to the question
What observations would (" strongly ") verify this statement?
need not be in terms of any observations of mine. It is reasonable
to assert that practically untestable hypotheses are not worth
bothering with. It is reasonable to assert that every significant
sentence must describe a " possible " observation (in a wide
sense of " possible "—but however wide it is, it will still suffice
to cut out a good deal of " metaphysics "—e.g. " The Universe
is a single substance " or " The world of sense is unreal ").
It is reasonable to concentrate attention on the problem of how
· hypotheses are verified, and to distinguish between the different
sorts of verification appropriate to different sorts of propositions.
It is not reasonable to suppose that this is the only sort of philoso-
phical problem worth considering. And it is not reasonable
to call it " analysing the meaning of propositions "; for this is
the natural description of another sort of problem.

ON THE " JUSTIFICATION " OF INDUCTION [1]

By Casimir Lewy

I SHALL discuss the problem of the justification of induction by reference to some passages in Mr. Russell's " Problems of Philosophy." He states the problem as follows (p. 101) :

"The question we really have to ask is ' When two things have been found to be often associated, and no instance is known of the one occurring without the other, does the occurrence of one of the two, in a fresh instance, give any good ground for expecting the other ? ' ... It must be conceded, to begin with, that the fact that two things have been found often together and never apart does not, by itself, suffice to *prove* demonstratively that they will be found together in the next case we examine. The most we can hope is that the oftener things are found together, the more probable it becomes that they will be found together another time, and that, if they have been found together often enough, the probability will amount *almost* to certainty. ... Thus probability is all we ought to seek."

I am going to ask the very same question : What *justifies* us in passing from a premiss of the form " A and B have always so far as we know been associated " to the conclusion of the form " A and B will *probably* be associated in the next instance " or to the conclusion of the form " Probably A and B are *always* associated"?

It should be noticed that I've stated inductive conclusions in terms of *probability*. I don't want to discuss in this paper whether or not inductive conclusions are *always* only more or less probable and never *absolutely* certain ; what I am going to do is to assume that at least *some* inductive conclusions are stated in terms of probability (i.e. are of the form " It is more likely than not that .." " It is probable that ... " " It is very highly probable that ... " etc.). Whether or not we claim only probability and never absolute certainty for all inductive conclusions (i.e. whether or not some inductive conclusions are of the form " It is absolutely certain that ... ") there can, I think, be no doubt whatever that there are a great number of them for which this is all that we claim. It is the purpose of the present paper to consider the question

[1] The sole purpose of this short paper is to introduce a discussion. I cannot, therefore, develop my view as fully as I should like to do.

of the justification of such conclusions. I think this is the question which many philosophers like Mr. Russell, Mr. Keynes and Professor Broad have tried to answer.

To go back to Mr. Russell, if I understand him correctly he claims that inductions can only be justified if we *assume* something which he calls " the inductive principle." This " principle " is formulated in two ways, but I shall only quote the first formulation :[2]

" (a) When a thing of a certain sort A has been found associated with a thing of a certain sort B, and has never been found dissociated from a thing of the sort B, the greater the number of cases in which A and B have been associated, the greater is the probability that they will be associated in a fresh instance in which one of them is known to be present ;

(b) Under the same circumstances, a sufficient number of cases of association will make the probability of a fresh association nearly a certainty and will make it approach certainty without limit."

He goes on to say :[3] " All arguments which, on the basis of experience, argue as to the future or the unexperienced parts of the past or present, *assume* the inductive principle ; hence we can never use experience to prove the inductive principle. Thus we must either accept the inductive principle on the ground of its intrinsic evidence, or forgo all justification of our expectations about the future. If the principle is unsound, we have no reason to expect the sun to rise tomorrow, to expect bread to be more nourishing than a stone, or to expect that if we throw ourselves off the roof we shall fall . . . All our conduct is based upon associations which have worked in the past and which we therefore regard as likely to work in the future ; *and this likelihood is dependent for its validity upon the inductive principle.*"[4]

In so far as I understand it, Mr. Russell's view amounts to this : that no inductive argument is justified, *unless* we assume that " the principle of induction " is true. We can neither prove nor disprove it by experience, nor yet is it an a priori proposition, but we have got to assume it, because otherwise almost all the arguments which we use in science and ordinary life would be without any justification.

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[2]p. 103.   [3]p. 106.   [4]The italics are mine.

Let us try to see what exactly this view comes to when applied to a particular case.

The example I am going to take consists of two premisses and a conclusion.[5]

The premisses are :

(1) Whenever I have heard barking in the past there was always a dog somewhere near; and

(2) I am hearing barking now; and the conclusion is :

(3) I have good reason to believe that there is a dog somewhere near now.

As I interpret Mr. Russell's position, he claims that (1) and (2) do not entail (3) ; i.e. that (3) does not follow from the conjunction of (1) and (2), *unless* we assume a further premiss to the effect that (1) and (2) do entail (3). That is to say, he maintains that the premisses (1) and (2) do not by themselves entail " I have good reason to believe that there is a dog somewhere near now," unless we add something like " (1) and (2) do give a me justification for holding (3)."

This is the only meaning I can assign to Mr. Russell's statement that all arguments which, on the basis of experience, argue as to the future or the unexperienced parts of the past or present, *assume* the inductive principle.

Now I think that any view on which in order to justify (3) we have to add a further premiss, or make an additional asumption, or accept a special postulate is completely mistaken. For according to me, the two premisses which I've stated *do* by themselves entail our conclusion.

In other words I claim that the proposition " Whenever I have heard barking in the past there was always a dog somewhere near and I'm hearing barking now " *entails* the proposition " I have good reason to believe that there is a dog somewhere near now."

Why do I say that (1) and (2) entail (3) ? Simply because it seems to me that the following proposition " Whenever I have heard barking in the past there was always a dog somewhere near, I'm hearing barking now, but I have no reason whatever to believe there is a dog in the neighbourhood " is *self-contradictory*. I can not prove that this is so, but I should ask you to reflect on how we *use* expressions like " I have good

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[5]This example is due to Professor Moore.

reason to believe," " It's probable," " It's very likely " etc.

If you reflect on how these expressions are actually used, I'm sure you will see that the proposition I've just stated is self-contradictory in the very same way in which it is self-contradictory to say " All men are mortal, Smith is a man but Smith is not mortal."

Thus, to put it very briefly, I claim that inductive conclusions follow from inductive premisses in the same way in which syllogistic conclusions, say, follow from syllogistic premisses. In other words, I claim that inductive conclusions *logically follow* from inductive premisses, which is another way of saying that inductive premisses *logically entail* inductive conclusions.

If I am right in this contention, as I think I am, it becomes at once apparent that no " principle of induction " is required for the justification of arguments " which on the basis of experience argue as to the future or the unexperienced parts of the past or present."

Professor Broad in his paper on " Mechanical and Teleological Causation "[a] also talks about justifying induction by means of accepting a special non-empirical principle or postulate. Such a justification is necessary because according to him "it's quite certain that if my *only* premiss is ' I've observed N ϕ 's and 100 per cent. of them were ψ there is no valid form of argument by which I can either prove or render probable the conjecture that 100 per cent. of the ϕ's in nature are ψ ". If my view is correct then far from its being certain that this is so, it is certain that this is *not* so. It is quite certain that there *is* "a valid form of argument" by which starting from Broad's premiss we can render probable his conclusion. Consequently his dilemma that either he does know some principle which in conjunction with suitable empirical evidence would justify him in making this conjecture, although he cannot elicit or formulate such a principle ; or no empirical evidence however regular, varied, and extensive gives him the slightest ground for believing any inductive conclusions, does not arise at all.

[a] Arist. Soc. Supp. Vol. XIV p. 87.

Cambridge,
June, 1939.

ANALYSIS

SPECIAL ANNOUNCEMENT

The editor offers his apologies for the lateness of the present number, which has been delayed by the need for discussing war-time plans.

Analysis will continue to be published, in a slightly altered form. Volume 7 will probably consist of three numbers, of which it is hoped that the first will appear before the end of November. The total number of pages in the volume will approximate to that in previous volumes. Each number will cost 2s. ; the annual subscription will still be 10s. 6d. post free for members of the Analysis Society and 6s. post free for non-members. Should unforeseen difficulties of production arise after the first number has appeared, the publishers reserve the right to complete the volume in one further number : but every effort will be made to produce three numbers.

Readers will realize that to maintain a journal of the character of *Analysis* in time of war is far from easy ; and they are accordingly urged to give it their fullest possible support. Subscriptions for the next volume should be sent in without delay : an order form is enclosed with this number. Readers who are not members of the Analysis Society are asked to become members of the Society if they can possibly afford to do so. Financial support, through subscriptions to the Society, is essential to the existence of *Analysis* in peace-time ; still more so in war-time.

Readers in the United States of America are specially asked to help. They can do so by subscribing as members of the Analysis Society rather than at the ordinary rate, or by sending a donation; and by persuading their friends to subscribe.

It is hoped that readers who have leisure for writing will also help by submitting papers for publication.

Subscriptions and applications for membership of the Analysis Society should be sent to Basil Blackwell, 49 Broad Street, Oxford. All other communications relating to the Analysis Society should be sent to the secretary, C. H. Whiteley, The University, Birmingham 3.

THE ANALYSIS SOCIETY

A meeting of members of the Analysis Society was held
during the Joint Session of the Mind Association and Aristo-
telian Society at Edinburgh in July.   Suggestions for a further
discussion meeting were invited, and as a result the following
arrangements have been made. A meeting will be held if possible
on 6 and 7 January, 1940.   The subject will be ' Induction ' :
one paper on this subject appears in this number of *Analysis*.
Members of the Society are invited to submit further papers on
' Induction,' but no guarantee can be given that all papers
submitted will be used.   Papers by non-members of the Society
may also be considered.   Those who wish to submit papers are
asked to communicate with the editor of *Analysis* or the secretary
of the Analysis Society without delay.

Further particulars will be announced later. Careful considera-
tion will be given to the choice of the place of meeting : it has
been suggested that Cambridge might be suitable.

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